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ABSTRACT

This paper presents three different aspects of research in industrial and organizational psychology. First, characteristics of major advances in science, and in the social and behavioral sciences are given, including: (1) team research is more common for major advances; and (2) young men under 35 are responsible for many major contributions. Secondly, the characteristic ways industrial and organizational psychologists conduct their research and the research requirements in these areas for the seventies are considered, including: (1) standardization; (2) replication; (3) more funding; (4) more valid instrumentation; and (5) measured response to "innovation". Third, considering the above two areas, the optimum approaches of industrial and organizational psychology research to meet the requirements of the future are explored. Examples included the setting up of a series of panels to serve as standard-setting groups, problem solvers, links between the academic and non-academic researchers, and to perform functions which are described. (KJ)

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RESEARCH MODELS OF THE FUTURE FOR INDUSTRIAL AND ORGANIZATIONAL PSYCHOLOGY[1]

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In this paper we shall look at three different aspects of research in industrial and organizational psychology. First, what are the characteristics of major advances in science, and in the social and behavioral sciences in particular. Second, what are the characteristic ways industrial and organizational psychologists conduct their research, and what are the research requirements in these areas for the seventies. Third, considering the above two areas, what are the optimum approaches of industrial and organizational psychology research to meet the requirements of the future.

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CHARACTERISTICS OF ADVANCES IN SCIENCE

Recent analyses of major advances in social science since 1900 indicate a number of trends which should enter into our thinking about optimum research models for the future. There are nine findings which are of special interest (the first six from Deutsch, Platt, and Senghaas, 1970):

1. Team research is increasingly common for major advances.
2. Advances often result from systematic research in an interdisciplinary center.
3. Major advances usually involve new theory, methods, and empirical results.
4. Young men, often under 35, are responsible for many major contributions.
5. Advances are requiring more human and economic resources.
6. Practical demands have inspired about four-fifth's of the recent advances.
7. Productive fields of research have "invisible colleges" which rapidly disseminate information among their members (Price, 1965a).
8. Knowledge is accumulative, one study building on another (Price, 1963; 1965b).
9. Research paradigms are important to the scientific advance of a field (Kuhn, 1962).

In what will follow, the assumption is made that these factors, important for other sciences, also are important for industrial and organizational psychology. And yet, as this paper will show, the items above seem to be honored more by their absence then by their presence.

CHARACTERISTICS AND REQUIREMENTS OF RESEARCH IN INDUSTRIAL AND ORGANIZATIONAL PSYCHOLOGY

There have been many recent criticisms of psychology and behavioral science (Bixenstine, 1964). Perhaps the most recent and telling critic has been Sigmund Koch (1969). He appraised psychology as a field which after many years of research and effort has made relatively little progress in understanding human behavior. He goes so far as to advocate the breaking up of the discipline of psychology into its separate components, with psycholinguistics, for example, going to the linguistics department and physiological psychology going to the physiology department. The time is ripe for reassessment of our own field of industrial and organizational psychology to see if we are making a meaningful contribution.

While psychology as a discipline has a history going back over a hundred years, industrial psychology as a research field has a much shorter history. In 1918 only one member of the American Psychological Association reported industrial psychology as his research field. By 1937 the number had increased to only 34 (Fernberger, 1938). Presently, 237 psychologists report basic or applied research in industrial psychology as their main activity (Cates, 1970). As in many scientific fields, the vast majority of all those who have contributed to industrial psychology are still living (Price, 1965b).

Required for the Seventies: Standardization and Replication

From 1954 onward, the cry of the industrial psychologists was for more theory (Lanier, 1954). In the Sixties we saw the development of a number of micro theories for studying men at work. For the Seventies, I believe the problem is no longer lack of theory, but lack of standardization. Let us take an example from the area of attitudes and job satisfaction. It is estimated that there are over 4,000 studies in this area and yet it is extremely difficult for us to state generalizations concerning the

relationship between job satisfaction and other areas of man's productive life (Locke, 1969). Why is this so?

Mukherjee (1969) shows that there is little correlation between the various measures of job satisfaction. (Almost 40 years ago, Stagner (1932) reported the same thing for personality tests as did Viteles (1922) fifty years ago for intelligence tests.) This means that when one investigator says he is measuring satisfaction toward work and its relation to productivity while another investigator says that he is also measuring the same concepts with different instruments, if the results differ we do not know whether this is because there is no relationship between "job satisfaction" and productivity or because the instruments are measuring different aspects of behavior. Why has there been a proliferation of job attitude questionnaires?

I put the blame on the "immortality syndrome." It appears that each professional feels the need to devise his own job satisfaction questionnaire and place his name upon it. While this may insure brief immortality in the literature, it is counterproductive for the field as a whole. Also operative in the ubiquitous "innovation syndrome" is the attitude that you cannot replicate someone else's work because this would be considered uncreative, second-rate research.

How different it is in other fields, In physics, for example, there is often replication and verification of other's work. We often read about the verification of work in Russia by United States physicists, or vice versa.

Reluctance to use someone else's technique in industrial psychology can be seen in the recent debate over the Herzberg two-factor theory. The most vocal critics of the theory did not replicate Herzberg's interview

technique, but immediately attempted to disprove the theory by using entirely different, sometimes ingeniously-devised attitude questionnaires. A logical approach might have been, first, to replicate the original interview procedure; then, to extend it by using other questionnaire techniques and populations. In this manner two things would have been accomplished: First, an exact replication would have indicated if there was any use in carrying on the research, since the previous results might have been unique to the specific case, or dependent upon the content analyses procedure or the interviewing procedure of the specific investigator. Had the results been different, non-reproducibility would have been shown. If the results had been similar with the same interview technique, then the investigators would have been on firm ground to use a different technique with an equivalent population to see whether the findings were dependent upon methodology.

Required for the Seventies: More Funding

Koch makes a point of saying that psychology has had a great deal of money invested in it. My contention would be just the reverse, at least for the field of industrial and organizational psychology and for the instrumentation used by psychologists in general. Let us take an example from the accident field. Industrial psychologists interested in driving behavior have advocated for years the use of unprogrammed simulation as a technique for studying individual reaction patterns to accidents. During this time, the Federal Government has supported only one unprogrammed engineering test model of driving simulator, and this at a cost only slightly in excess of $100,000, an insignificant sum compared to the millions spent on atomic accelerators used by physicists (Barrett, 1967).

For even a small industrial organization, a capital outlay of this magnitude could hardly be called major. So, indeed, my conclusion is quite the reverse of Koch's. Very little money has been spent on equipment or needed instruments to help the psychologist study this critical problem of our society.

Required for the Seventies: More Valid Instrumentation

There is probably another syndrome operating here among psychologists, namely the idea that progress can be made by the psychologist acting alone as the instrument of prediction. In almost all fields of science, progress has been made by the better utilization of instrumentation and equipment to study the phenomena in question. Industrial psychologists have been slow to perfect and use sophisticated instrumentation and other hardware. This "clinical intuition" syndrome must be overcome. We know that, unaided by equipment, the physicist, biologist, or chemist did not progress very far. It is doubtful whether the industrial psychologist will make great strides either, unless he becomes more sophisticated in the use of both instrumentation and equipment to study men and organizations. It is really inconceivable to think of a "training organization" with instrumented production records whose main function is to serve as a laboratory for increasing our knowledge of men at work? There are training hospitals for the medical and nursing professions.

Even when the psychologist does use even relatively simple research equipment, the rigor of science is by-passed for a more glamorous investigation. This can be illustrated by an example from one of industrial psychologist's earliest concerns.

Approximately 35 published studies in the area of driving simulation have appeared since Munsterberg's (1913) work in the early 1900's. Of these,

only one recent investigation empirically related simulated driving behavior to real world driving behavior (Barrett, 1970). This one investigation comparing road and simulated driving performance showed no relationship; further, there was a nonsignificant relationship between driver performance on various simulators supposedly measuring the same behavior (Edwards, Hahn, and Fleishman, 1969). Still, investigators continue to study the effects of alcohol and more recently of marijuana with so-called driving simulators (Crancer, Delay, Dille, Wallace, and Haykin, 1969). One wonders how much acceptance this research would obtain if more appropriately titled "The effect of marijuana upon a task superficially resembling automobile driving."

Required for the Seventies: More Involvement in Large Programs

In contradiction to Koch's statements, we have tried to show that the low scientific output of psychology, especially of industrial psychology, is not concommitant with a great deal of time and money spent in the area; rather that little time and money has actually been spent in terms of the problems involved. Even where large blocks of time, effort, and money have been applied to a problem in our field, programs have often been selected which are counterproductive.

The best example is the Zero Defects Program sponsored by the Department of Defense. This program has been implemented in the major corporations in the United States, and millions of dollars are spent each year both in administering the program and in preparing its various promotional packages. Despite the millions spent in the implementation of the program it has no solid theoretical base and, more important, there has been no scientific investigation of its effectiveness. Indeed, much of the available evidence from attitude surveys is negative (Barrett and Cabe, 1967).

This is clearly an area where industrial psychology should have an impact but does not. It is a clear example of poor resource allocation. If one-tenth of even one-hundredth of the money had been spent for scientifically designed motivation programs, tried out in comparable locations, a major breakthrough might have been achieved in the area. Instead, the available resources have been dissipated on a program which is of dubious economic value and of practically no scientific worth.

In a comparable way, in 1965 the Advertising Council began a safety campaign that by 1966 had cost 40 million dollars in air time alone, but nothing had been spent to evaluate the effectiveness of the campaign (Haskins, 1970).

We should continually remind ourselves that society spends vast sums on projects which are important and need our expertise. Too often we are not involved in projects which do have large resources available to them.

Required for the Seventies: Greater Concern for Strong Relationships

In our field, we often display behavior in our publications which reveals some important basic attitudes. Thus, can anyone imagine a medical researcher reviewing the effect of various drugs upon the cure rate for cancer by merely grouping the drugs into categories and failing to indicate those which are most and least effective? Yet we do this in industrial psychology.

Consider Ghiselli's (1966) book on Validity of Occupational Aptitude Tests. There the tests are grouped into categories, and average validity coefficients are computed for various groups of occupations. From the way the information is presented, there is no way to determine which specific test is most predictive for any job classification. Unfortunately, also the references were omitted.

As a field we seem to ignore the need to look for strong relationships that can be replicated. Our failure to do so is underscored by the tone of reviews of projection tests which stress their lack of suitability for industrial use because of low validities (Guion, 1965; Guion and Gottier, 1965). Nevertheless Miner (1960; 1961; 1962), with a single specific projective test and scoring method, reported concurrent and predictive validities above .50. A more appropriate review statement would have been that one projective test showed great promise, and other researchers should attempt to replicate the findings.

Required for the Seventies: Better Dissemination of Research Results

We also have our invisible colleges. I was struck by this when preparing a recent review on comparative management surveys. Over 20 per cent of the references cited were prepublication nonarchival papers (Barrett and Bass, 1970). It is difficult to emphasize how serious this problem is for young researchers, perhaps at the height of their abilities, who are not yet members of such communications networks.

There is a proliferation of journals in the behavioral and social sciences. Journals of interest to psychologists number close to 1,000 (Garvey and Griffith, 1964). Research relevant to our specific concerns may be found in a variety of journals. In a count of the different journals referenced in chapters dealing with personnel, organizational, engineering, and consumer psychology in a recent text (Bass and Barrett, 1971), it was found that some 20 different journals were referenced in each chapter. As might be expected, some journals were always cited, such as the Journal of Applied Psychology, but there are many different journals where occasional articles of interest and value to our field appear.

There are undoubtedly also many technical reports and other nonarchival reports from industrial organizations, advertising agencies, and consulting firms which might make a significant contribution to the field.

As an illustration of this point, a well-designed study in a major industrial organization clarified some of the problems encountered in zero defect's type programs (Miller, 1964). Despite the investigation's relevance for other organizations, the report has not been widely disseminated nor appeared in archival form as far as I know.

I suspect the reason more technical reports by nonacademic psychologists do not become archival is because the data has been, in Etzioni's (1969) terms, "semi-processed." The depth and kind of analysis required for most industrial organizations is typically much less than that required for archival publication.

One partial solution might be more extensive use of special purpose data banks where organizations can deposit results of studies in order that others can later do more extensive analysis. We already have a few data banks in certain areas which serve this function (Barrett, 1969; Campbell, 1969; Schoenfeldt, 1970).

Required for the Seventies: Increased Research From Nonacademic Psychologists

There has been a shift in the Sixties from the academic to the professional. Haire in 1959 indicated that over half of the industrial psychologists were in academic institutions. Now approximately 25 per cent are in academic institutions (Cates, 1970). This means that more and more of the research in the field will be done by industrial psychologists who work in private organizations.

There is a basic anomaly which may be unique in our field. That is, the nonacademic industrial psychologist usually has the most resources, and is in the best position regarding subjects, to perform productive research. Unfortunately, the nonacademic industrial psychologist also has less time, either for research or to remain up to date on the most recent theoretical, methodological, and empirical advances in the field.

Required for the Seventies: Capitalize Upon the "Opportunistic" Nature of Our Research

Most other fields of psychology have a captive population, be it rats, college sophomores, or patients, with which to conduct their research. This is not true of industrial psychology.

The industrial psychologist by the nature of his research population is probably the most "opportunistic" group in the American Psychological Association. This is because if we are interested in studying people in real work situations we usually have to convince an organization there will be some ultimate economic advantage to the organization as a result of our research. The industrial psychologist usually has access to the employees of an organization when there is a problem. We should accept the "opportunistic" character of much of our research and attempt to capitalize upon it as much as possible.

Required for the Seventies: Measured Response to "Innovations"

The industrial psychology field is unique in that behavioral science "innovations" can be sold to organizations with little or no demonstration of their effectiveness. The "evidence" presented for the effectiveness of many "innovations" has little scientific worth. There have been, and I am sure will continue to be, "innovations" proposed and introduced with inadequate supporting evidence.

Examples of "innovations" which the field must react to are:

1. Office landscaping
2. Zero defects
3. Organization man or other-directed man
4. Time-span of discretion
5. Music on the job
6. Handwriting analysis for selection
7. Management by objectives
8. Managerial grid

I am sure you could add many more to this short list.

The question the field must decide is how much time and effort should be consumed in testing the "innovation" proposed and implemented by others. Obviously, there is an opportunity cost since the time spent by a researcher to test someone else's "innovation" might be better spent on his own research topics.

Required for the Seventies: Examine "Received Doctrine" More Closely

Haire (1959) pointed out that industrial psychology has always drawn from the theories and work of psychology. This is a mixed blessing. A theory may take on the characteristics of a "received doctrine" and become an unanalyzed article of faith (Means, 1965). This has been true of our acceptance of the validity of Weber's Protestant ethic theory (Means, 1965). A more recent occurance has been our uncritical acceptance of Maslow's hierarchial theory of motivation (Maslow, 1943). There is almost no empirical evidence for the theory in the psychological literature and that presented by Maslow (1950) would not be considered by many as very conclusive. No one would deny that the theory has had a great deal of influence upon both theories and research studies in industrial psychology (Alderfer, 1969).

Despite the wide attention accorded the theory, it required 25 years before the theory was empirically tested and found wanting (Hall and Nougaim, 1968; Beer, 1968; Braun, 1969). It is clear that until recently Maslow's theory has been accorded the status of "received doctrine."

Another theory which has to a degree been accepted in this way is McClelland's (1961) concept of motivation. In this case the supporting evidence for the theory has been strong and researchers have found evidence to support McClelland's concept (Andrews, 1967; Cummin, 1967; Wainer and Rubin, 1969).

The lesson seems to be that we should critically examine our theories, and leave "received doctrines" to other, non-scientific areas of endeavor.

Required for the Seventies: Focused Research on Paradigms

We as a field have curious publication patterns (Franke, Deep, and Barrett, 1970). As in many other fields, we have many "one-shot" studies. What is different is that in examining the recent literature of industrial psychology we found at best a single paradigm. The word, "paradigm," is employed in the sense in which Kuhn (1962) refers to theories or models of research which generate investigation. Our examination of the literature in our field led to the conclusion that industrial psychology is in the pre-paradigm stage of science. The fact is that relatively few constructs generated enough studies to even approach the paradigm level in industrial psychology. The closest we come to a paradigm in the recent literature is Herzberg's (1966) controversial two-factor theory.

An important question for our field is why the Herzberg theory should generate over twice as many publications than did Fiedler's (1967) theory.

Is it because Herzberg's theory:

1. Was easier to understand?
2. Was easier to sell?
3. Had greater theoretical significance?
4. Had greater promotional significance?
5. Was easier to criticize?

These questions should be answered, since we have devoted a great deal of professional energy to some potential paradigms while ignoring others.

Required for the Seventies: More Systematic Research

Our inability to do systematic research is illustrated by studies attempting to discover the relationship between personality and product

choice. There have been approximately 45 studies since 1932 in this complex area. Usually, each investigator chose a personality measure different from that of other studies, with the most popular, the Edwards Personal Preference Schedule, used in only four studies. Almost all the studies were "one-shot" investigations (Barrett and Whittaker, 1970).

There were two exceptions. The well-known Haire (1950) shopping list study was successfully replicated, but 20 years later, by Webster and Von Peckman (1970). The second exception was the continuous reworking and reexamination of the widely quoted Evans (1959) study of personality and automobile choice. Investigators went back to Evans' theoretical model and data until they exhausted its potential (Kuehn, 1963; Marcus, 1965; Evans, 1968; Evans and Roberts, 1963).

At this point, I am going to take what many may perceive to be a radical or dangerous step, and that is to suggest specific steps which we in our discipline can take to increase our research productivity.

ACTION STEPS FOR INDUSTRIAL AND ORGANIZATIONAL PSYCHOLOGY

The analysis given above leads us to a number of steps which industrial and organizational psychology can adopt to improve its effectiveness. The Scientific Affairs Committee of American Psychological Association Division 14 should be asked to set up a series of panels for the various topic areas of industrial psychology. These panels would serve as standard-setting groups. They would function much as do the various standard groups function in engineering or accounting. Among other things, these panels would be expected to define operationally "turnover," to set the standard methodology for measuring overall job satisfaction, and so on. These panels would

operate analogously to their counterparts in the field of engineering. Thus, when an engineer talks about lumens or decibels, this refers to an established standard which has been agreed upon in a professional society. The concept has common meaning, since a standard technique was used for measurement. When we talk about absenteeism and turnover, what do we mean? What should we mean? I feel that when we talk about unavoidable turnover, it should be measured in the same standard way in all research investigations. In the same manner, when we talk about supervisory ratings of employee performance on a scale of overall effectiveness, this could be a standard scale agreed upon by a representative group of industrial psychologists. If we are studying job satisfaction in general do we measure it with a "faces" scale or a rating form? While this violates both the "immortality syndrome" and the "innovation syndrome," it is probably the only way we are ever going to be able to talk in an efficient fashion about the relationships of interest to us. This does not preclude multiple measures of the same construct, but the nonstandard measures could be discussed and understood relative to the agreed-upon definition.

The panels could also act as central clearing houses, maintaining a file of ongoing research in each area. Periodically, they could be expected to analyze both past research and ongoing research, and to recommend areas which need further exploration or state where enough work had already been accomplished. New programs such as "management by objectives" could be identified early so that the evidence for or against the concept would be evaluated (Carroll and Tosi, 1969).

The sociology of science indicates that certain men have the ability to be problem finders, and can indicate problems of fundamental importance

(Merton, 1968). It would be hoped that the panel in each area would enlist the aid of such men, and indicate what they believed to be the important problems in the area.

Most scientific fields require review articles after each 30 to 40 papers to consolidate the knowledge from earlier papers that is thought to have been lost from sight (Price, 1965a). One of the panel's functions would be to indicate the need for a review article in a sub-area.

If this system had been in effect, possibly the multitude of dissertations and articles dealing with the Herzberg controversy could have either been avoided or taken a more systematic direction.

The panels should also act as links between academic and nonacademic researchers. An academic researcher may have a theoretical proposition for which empirical data is required. For example, an academic researcher may want to test Kornhauser's (1966) "spillover" interpretation of the relationship between job and life satisfaction by using two groups which are similar in job function but quite different in terms of job satisfaction (Iris and Barrett, 1970). The psychologists in an organization may know of an "opportunity" to study two groups with these specifications. A collabrative research effort might result. This, in effect, would be a more systematic way to capitalize upon the "opportunistic" nature of much of our research.

The panels would indicate where <u>replications</u> were required as warranted by the research evidence. Hopefully, in this manner, the field will generate a body of verified factual information.

The panels would have an unique opportunity to obtain extremely rapid dissemination of their findings by utilizing the <u>Experimental Publication</u>

System. In this manner, the "invisible college" would be open to all who were interested. Both the most experienced and the newest researchers would have equal access to information.

Each panel would be encouraged to explore the possibility of establishing a computed-based data bank, with industrial organizations being encouraged to deposit "semi-processed" study results. For example, the panel on job attitudes might decide to build a data file containing results from the Job Description Index, and encourage users to deposit their results in the bank along with selected biographical, organizational, and environmental data.

By implementing these steps, a number of practical results should follow. First, collabrative research should be fostered, since those with common research interests could readily be identified. Second, overall, more systematic research should be conducted, since those working in an area will have more up to date information about the progress of others. Third, the younger men in the field will have the same access to the "invisible college" as the more experienced researcher. Fourth, the resources of the academic and industrial community of researchers can profitably be joined. For example, the typical industrial psychologist may be called upon to conduct an attitudes survey, but he may not be particularly knowledgeable regarding the recent literature and current methodological debates. In order to focus in on the key issues, he can, of course, survey the literature himself, but this is often a time-consuming and frustrating process, since there are increasingly large numbers of studies being generated and he has no way of knowing which are the most critical, especially if he has not previously worked in the area.

The industrial psychologist in an organization may have access to a population and a problem which could fit well into the scientific study requirements of the field. In this way, the direction of the field could be given some focus and the psychologists in the industrial situation would have more opportunity to make a meaningful impact in the field and in solving organizational problems. Fifth, nonproductive areas, or areas where enough negative evidence has accumulated, would be pointed out to researchers. For example, there are many superfluous "innovators" in the behavioral science field. As a case in point, Jacques (1964) has advocated the concept of "time-span of discretion." This technique of job evaluation is claimed to decrease dramatically the time required in job evaluation, and also to be well accepted by employees. The independent evaluations in the literature, however, show either the opposite or mixed results (Kidd and Barrett, 1970). The question, now that we have negative evidence on the concept, is how much more negative evidence is required before the concept is rejected by the discipline as unprofitable.

What is required is an overall look at the field by professionals who can see the pattern of research. Perhaps at this point in time there is enough negative evidence concerning time span of discretion that more would be flogging a dead horse. The same might have been said years ago of the Herzberg controversy. Perhaps the first 20 negative articles would have been enough, and a new direction should have been proposed. In terms of the maximum utilization of the available manpower, if a committee had been established to follow trends in industrial and organizational psychology and to make recommendations, Ph.D. students might have been well advised of another subject than the two-factor theory. There is an opportunity

cost here which the Ph.D. student contemplating a dissertation topic, or his advisor, may not be able to forecast independently. This idea of a committee to indicate areas which may be overcrowded or substantially negative, beyond what is required for proof, and the suggestion of re-direction of effort, might make great sense for the field as a whole.

We in the field of industrial and organizational psychology are in a favorable position to make continued advances. Managers of industrial organizations are increasingly interested in what the behavioral sciences can offer (Rush and Wikstrom, 1969). We are a small enough group that we still can communicate readily with one another. My hope is that our field will not generate another 4,000 studies on job attitudes without enough standardization to allow knowledge to accumulate. Ominously, a new fad has already appeared on the horizon: Studies in "organizational climate." Each investigator has chosen to define the construct in his own special way. If we don't standardize on the meaning of this construct, 50 years from now it will be in the same morass into which job satisfaction studies have already sunk.

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